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ABSTRACT

What has been labelled mainstream dialectology has been criticized soundly on theoretical grounds, yet mainstream dialectologists have responded with the assertion that their critics have not been intimately familiar with dialect methodology and are therefore not qualified to criticize. Claiming that while theoretical issues are far from being settled, their data are nontheless valid, they insist that all of the U.S. be mapped by traditional linguistic atlases. This paper argues against the creation of a Linguistic Atlas of the Southwest States (LASS) and more broadly calls for a halt to any further work on the old Linguistic Atlas of the United States and Canada project begun in 1931. The point is not just that mainstream dialectology has flaws; the point is that the traditional work is so bad it is not worth continuing. Concentrating upon the data of mainstream dialectology (its supposed strength), the paper argues that the weaknesses are so great that the methodology is not salvageable. This refutation, however, does not mean that dialect research should not be conducted. On the contrary, research in language variation is vital, but wide-scale studies must abandon the methodology of mainstream dialectology and replace it with a new one embodying, among other improvements, some sociolinguistic sophistication. (Author)

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NEEDS IN SOUTHWEST ENGLISH DIALECTOLOGY OR, WANT A LASS? THANKS, BUT NO THANKS

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Workshop on Research Problems in
Southwest Areal Linguistics II
The University of New Mexico, Albequerque
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ABSTRACT

What has been labelled mainstream dialectology has been criticized soundly on theoretical grounds, yet mainstream dialectologists have responded with the assertion that their critics have not been intimately familiar with dialect methodology and are therefore not qualified to criticize. Claiming that while theoretical issues are far from being settled their data are nontheless valid, they insist that all of the U. S. be mapped by traditional linguistic atlases. This paper argues against the creation of a Linguistic Atlas of the Southwest States (LASS) and more broadly calls for a halt to any further work on the old Linguistic Atlas of the United States and Canada project begun in 1931. The point is not just that mainstream dialectology has flaws; the point is that the traditional work is so bad it is not worth continuing. Concentrating upon the data of mainstream dialectology (its supposed strength), the paper argues that the weaknesses are so great that the methodology is not salvageable. This refutation, however, does not mean that dialect research should not be conducted. On the contrary, research in language variation is vital, but wide-scale studies must abandon the methodology of mainstream dialectology and replace it with a new one embodying, among other improvements, some sociolinguistic sophistication.

NEEDS IN SOUTHWEST ENGLISH DIALECTOLOGY
OR, WANT A LASS? THANKS, BUT NO THANKS

I

Among those linguists Wolfram (1971:103) politely labelled "mainstream dialectologists," highest priority is given equally to two general tasks: (1) the editing and publication of data already collected by existing linguistic atlas projects (e.g. the Linguistic Atlas of the Middle and South Atlantic States, the Linguistic Atlas of the Upper Midwest, etc.), and (2) the establishing of linguistic atlases in the "unmapped" areas of the United States. Such priorities have clear implications for the Southwest. California is "mapped" by the California and Nevada section of the Linguistic Atlas of the Pacific Coast, begun in 1951 by David Reed when he was at the University of California at Berkeley (Reed and Reed 1972:135), and plans for the Linguistic Atlas of the Gulf States (LAGS), under the direction of Lee Pederson, extend the scope of that atlas as far west as the city of Laredo and Webb County, Texas (Pederson et al 1972:229, 233, 240). Assuming that Pederson and Reed can be relied upon to finish their respective projects, we would reasonably conclude that the mainstream dialectologists would say that highest priority in the Southwest should be given to an atlas of central and western Texas, New Mexico, and Arizona--in other words a Linguistic Atlas of the Southwest States (LASS). But do we want a LASS? My answer is, "Thanks, but no thanks."

When the ambitious Linguistic Atlas of the United States and Canada was inaugurated in 1931 by Hans Kurath, it was perhaps a noble project. But in the past 42 years linguistics has obviously advanced too far for us to continue to study dialects with a methodology that has not been significantly improved in almost a half a century. Despite Kurath's recent and repeated assertion to the effect that "there can be no valid objection to this procedure" (1972:26), there are indeed numerous valid objections to traditional American linguistic geography as it has been practiced by Kurath and his students for the past four decades. It may have seemed reasonable 42 years ago to import a dialect theory and methodology which European scholars had thought to be adequate for studying the language of a stable peasant society and to attempt to replicate successfully this type of research in American society,[1] but American innocence of the 1930's

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[1] About this point there can be little disagreement. Atwood writes: "The methodology of the American Atlas has been essentially that of Gilliéron, as refined and modified for Italy by Karl Jaberg and Jakob Jud" (1963: 7). Even Pederson asserts positively:

> The Linguistic Atlas of New England (LANE) Project combined the best features of its predecessors and established a standard of excellence for all of the American dialect research which has been done during the past three decades. Like Wenker and Wrede's project in Germany, Kurath's work became a model for regional research in the United States.... Positioned

is no excuse for untenable dialectology in the 1970's. But if we compare the first American dialect atlas, the Linguistic Atlas of New England (LANE), with the newest, the Linguistic Atlas of the Gulf States, it is clear that there is no significant change in the thinking of American linguistic geographers. With minor allowances, criticism of one atlas is applicable to any other.

It is not my point to argue that linguistic atlas methodology needs to be modified. My point is that the methodology needs to be abandoned and replaced by a new one if we are seriously interested in a realistic, accurate account of regional and social variation in American English. In my description of the research design of the Arkansas Language Survey (ALS) I said that the methods of the ALS were basically those of traditional dialectology which had been modified to take advantage of improvements and innovations developed in urban studies.[2] That is not merely a discreet understatement; it is essentially a false claim.

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squarely in the Gilliéron tradition, the LANE Project incorporated the central tenets of dialectology instituted in ALF (1972:8).

[2] Note that implicit in that statement was the confession that advances had not been made by dialect geographers (Underwood 1972:4).

An attentive reader of that paper knows very well that about all the ALS has in common with linguistic atlases is an interviewer interviewing a native speaker. To be sure, I am not the first to criticize American linguistic geography. Devastating criticism in more or less overt forms can be found in Pickford 1956, Keyser 1963, Labov 1966, C.-J. Bailey 1968a, and Dillard 1969, to mention just the most prominent critics. But the response among mainstream dialectologists has been either to procede as if no questions have ever been raised or to make minor adjustments as gestures of keeping up with the times. My own work until 1971 was no exception. My training was in traditional linguistic geography, and the character of my early works is unmistakably conservative. For a while I was preoccupied with modifying traditional methodology to overcome valid criticisms, but it soon became apparent that reconciliation is impossible. My present position is distinctly different from that expressed in Underwood 1970. I am not advocating reform; I am advocating repudiation. It is my contention that if we objectively examine the linguistic atlas methodology, focusing our attention on aspects ignored by previous critics as well as on a review of prior criticism, the only reasonable conclusion is that the methodology is not salvageable.

American linguistic geography has been perceptively criticized on theoretical grounds in such works as Keyser 1963, C.-J. Bailey 1968a, Labov 1966, and Troike 1971.

Furthermore, generative and lectological alternatives to traditional dialect interpretations are well documented. In addition to the works just cited, see also B. Bailey 1965; C.-J. Bailey 1968b, 1972, 1973a, and 1973b; Becker 1967; King 1969; Labov 1969, 1972a and 1972b; Labov, Cohen, Robins, and Lewis 1968; Loflin 1967, 1969, and 1971; O'Neil 1968; Saporta 1965; Sledd 1966; Smith 1969; Thomas 1967; Underwood 1971; and Weinreich, Labov, and Herzog 1968. Rather than to replow that fertile theoretical ground, for the sake of brevity I wish to set aside consideration of analytical procedures in deference to those who claim that linguistic atlas data can be interpreted within the construct of any theory and to ask some fundamental questions about the data themselves. (The preceding sentence must not be interpreted to suggest that I subscribe to the claim; on the contrary, I think it is without validity.)

Typically empiricists to the extreme, American linguistic geographers generally eschew discussions of dialect theory or hypotheses. Indeed, it is no accident that the first chapter of Kurath's Studies in Areal Linguistics is titled "From Sampling to Publication" or that the word theory is not to be found in the exhaustive table of contents of this book (Kurath 1972:v-vii). A look at Kurath et al 1939, Kurath 1949, Atwood 1953, or Kurath and McDavid 1961 reveals that nowhere do the authors offer an explication of the theoretical framework of the studies. Typically the dialect geographer begins in the first chapter of his book by outlining

dialect areas or by summarizing dialect differences without bothering to explain his conceptualization of dialects or his motivation for using some linguistic features instead of others to determine dialect differentiation. Even the two articles often cited as the best state-of-the-art papers, McDavid and McDavid 1956 and Atwood 1963, ignore dialect theory while describing field methods in exhaustive detail. But if we look closely at the methodological statements, we can find in them countless unstated assumptions and untested hypotheses, and close interpretative reading will even reveal a fantastically "strong" theory of dialect.

Support for the assertion that mainstream dialect methodology is untenable can be provided by considering four basic questions: (1) What questions do linguistic geographers ask? (2) How is information elicited from the speakers? (3) What kinds of Americans are interviewed? (4) In what kinds of communities are interviews conducted?

The Questionnaires

Because of the linguistic geographers' concern about comparability of data from one atlas with data from another, every American atlas uses a questionnaire developed from the original LANE work sheets but tailored in some ways to suit the locale.[3] Curiously, despite the dialectologists' pre-

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[3] See Davis, McDavid, and McDavid 1969 for a compilation of items used in American atlas surveys.

occupation with comparability, which is one reason they adamantly oppose any important changes in their methodology, this concern is highly and capriciously selective. They insist that items used to mark dialects along the Atlantic coast be retained by all later studies. Thus, the LAGS interviewer in 1973 asks his informants their terms for a sheaf of wheat (Pederson et al 1972:141), just as the LANE field worker did in 1933 even though wheat may not be grown in, say, Zapata County, Texas, of if wheat does happen to be grown somewhere in the LAGS territory, to find it tied into sheaves would surely be a rare experience. On the other hand, comparability is not a concern when field workers use radically different elicitation techniques, when two sets of data are collected 40 years apart, or when responses are given in a wide variety of contextual styles.

Clearly, any dialect questionnaire must be selective, and even Kurath admits that the selection of items for inclusion presents "problems" and "uncertainties" (1972:3-4). But though there may be questions concerning individual items to be included, there are no questions in Kurath's mind about the types of data to include, for the questionnaire must "cover the regional and/or social differences in the lexicon, the morphology, and the phonology in a given language area..." (1972:3). Two assumptions in that statement should be obvious: (1) Dialect differences are confined to what people actually utter. There is no consideration of

the possibility that dialects may differ according to the acceptance or rejection of the hypothesized grammaticality of an utterance. (?) Kurath's catalog clearly disregards or rules out of consideration dialect differences in syntax. Actually there are a few syntactic matters in atlas work sheets, but they are largely confined to choices of prepositions and the use of double negatives. Several of these features are bizarrely treated as lexican items in Kurath 1949. In addition, the LAGS work sheets include deletions of articles, auxiliaries, and prepositions (Pederson 1972: 18). But linguistic atlases do not provide any insights into dialect differences in intonation patterns (a point Sledd has stressed repeatedly, e.g. 1969:1313) or differences in syntactic features such as embedded questions, subject-verb inversion in direct questions, relativization, complementation, or indirect object inversion. Even for all his concern for for phonology, the atlas dialectologist can tell us almost nothing about contraction and deletion phenomena, l-lessness, word-final consonant cluster simplification, and other consonant pronunciations. Consequently, the atlas work sheets are frankly unenlightening with regard to black-white speech differences and would be even less productive when used to elicit data on Chicano or Indian English.

The linguistic atlas work sheets have a severely limited usefulness because of their unmistakable Anglo-Saxon bias.

As early as 1928, three years before LANE was underway, Kurath had already concluded:

> The dialect differences in the pronunciation of educated Americans from various sections of the country have their origin largely in the British regional differences in the pronunciation of standard English (1928:394).

In view of this narrow historical account of American dialect differences, it is no surprise to discover that the kinds of evidence which atlases use as dialect markers are rigidly restricted. This limitation is revealed by Kurath's list of sources from which items were taken for use in atlas studies:

> The lexical and morphological features in the work sheets are selected from an extensive collection compiled largely from the regional and local word lists in Dialect Notes and American Speech; the phonetic items are derived mostly from H. Kurath's collection and from J. S. Kenyon, American Pronunciation (4th ed., Ann Arbor, 1930). Most of the dialect features in G. Kempl's short list (Dialect Notes 1, 316-18) have been included.... In order to facilitate comparison with British dialects, an effort was made to include as many features as possible from A. J. Ellis' Comparative Specimen, his Dialect Test, and his Classified Word List (Early English Pronunciation, part V, pp. 7*, 8*, and 25*-29*: London, 1889) (Kurath et al 1939:148).

In other words, items were chosen to indicate distinctions in American dialects because those items had previously shown to have regional variants among British or Anglo-American speakers of English. This selection in effect defined dialect differences as speech differences recorded within this narrow range of variables. Regardless of how extensive differences may be outside the domains of the

work sheets, they must be ignored. Therefore, it is easy to see why Kurath could conclude:

> By and large the Southern Negro speaks the language of the white man of his locality or area and of his education.... As far as the speech of the uneducated Negroes is concerned, it differs little from that of the illiterate white; that is it exhibits the same regional and local variations as that of the simple white folk (1949:6).

Disregarding the fact that Kurath's conclusion can be based upon the vocabulary of no more than 50 Blacks in the entire Atlantic coast region,[4] we can acknowledge that Kurath has some empirical evidence to support his conclusion. In response to atlas questions little or no differences may be found between Southern Blacks and whites, but that does not necessarily mean that dialect differences do not exist. It may mean only that atlas interviewers were--and still are--asking the wrong questions.

Consider, for example, the types of vocabulary items that mainstream dialectologists regard as relevant for determining dialect groupings. Topics covered by the atlas questionnaires include such subjects as the weather, the dwelling, the farm, farm implements and tools, domestic

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[4] The exact number is unclear. Atwood says: "The South Atlantic materials include records of 21 more or less primitive Negro informants gathered by Loman; McDavid has added several more [from South Carolina], including some examples of educated Negro speech" (1953:1, n 4).

animals, farm crops, clothing and bedding, and foods and cooking (Kurath et al 1939:149). Obviously, a person's entire vocabulary inventory cannot be studied, but the mainstream dialectologist needs to consider whether his selections provide an accurate representation of dialect differences. It is just not enough to acknowledge that his selections present "problems" and "uncertainties" (Kurath 1972:4). As Dillard has pointed out:

> ...of course there is nothing wrong per se in investigating the domain of the home or of the farm. There are, however, other domains which should not be ignored. It is particularly unfortunate if the decision about a cultural system becomes the artifact of the questionnaire applied, as has been done in observing that the Negro brought an "agricultural" life pattern to the inner city. It would not be surprising if it were found that Southern Negroes have the same terminology for farm implements (whiffletree, harrow) that Southern whites have. Southern farming developed, after all, with Negro labor but with white ownership of the land and of the implements. But it would be absurd to expect Negro musical terminology to be identical to that of the whites (1969:11).

A dialect questionnaire must not be so culturally biased that it distorts the evidence gathered from speakers whose culture differs from the norm.

But notice, too, that the vocabulary questions in linguistic atlas work sheets have a decidedly rural bias even though the American population is predominantly urban. Must this mean, then, that only rural items are legitimate dialect markers? Are we to conclude that urbanites and suburbanites who know only national variants or no terms for the rural

test items have homogeneous vocabularies? Obviously, the answer to each question must be negative, yet atlas data suggest that the answers are affirmative. But more importantly atlas questions are not just rural; they are slanted toward a rural life style that was already disappearing when LANE was organized. Kurath acknowledged that the decision in 1930 was to record the obsolescent vocabulary of pre-mechanical agrarian America before that vocabulary completely disappeared and could never be adequately known. LANE concentrated upon this vanishing vocabulary because:

> Such words reflect most clearly the regional pattern of pre-industrial New England, which must be reconstructed as well as possible if we could understand fully the present speech areas and trace the sources of New England speech back to the dialects of England (Kurath et al 1939:1).

Indeed, as Labov's recently-published study of the changes in the dialect of Martha's Vinyard demonstrates, the LANE questionnaire was oriented more to the 1890's than even to the 1930's (1972c:87,95-102). Despite this antiquarian bias, mainstream dialectologists insist that questions significant for the 1890's continue to be used (because of the comparability argument) in the 1970's. So Pederson's LAGS questionnaire still covers such items as calls to oxen, the parts of a wooden wagon wheel, shafts of a buggy, and razor strops (Pederson et al 1972:index).

There is still another problem with the atlas questionnaires. Atwood notes: "In general, the items on the list

represented everyday concepts, and sought to elicit usages that would ordinarily be transmitted orally within the family than in the schools" (1963:11). At first glance that statement appears innocuous enough, for it would seem obvious that dialect investigators are not interested in national usages disseminated through mass education. But on second glance the statement contains a powerful assumption about the transmission of dialect features. In the 1930's researchers focused upon parentally transmitted features because it was commonly assumed that the greatest influence on a child's speech is the speech of his parents. Today, however, we know that parental influence is not nearly so great as peer group influence in affecting language characteristics. Any study that concentrates upon features that are learned from parents and systematically ignores those learned from one's peers runs the great danger of producing a distorted version of dialect patterns that exist.

The family bias in atlas questionnaires is dictated by the linguistic geographer's insistence that the single most important factor in determining dialect groupings is migration patterns (Kurath 1949:1). The claim has been that as a family migrates, its dialect migrates with the family, and the features of that dialect are transmitted to the next generation in the new locale. Such a strong theory of dialect transmission quite often fails to explain such well known phenomena as isoglosses cutting across instead of

running along seams in migration routes. (See, e.g., Kurath 1949: maps 27-28, and Marckwardt 1957: maps 2-3.) Instead of opting for a weaker transmission theory, Kurath has chosen to patch up the original hypothesis by acknowledging that some dialect variants spread independent of migration because they disseminate as prestige forms (1964). But such an ad hoc explanation does not affect the important claim that children learn their language from their parents, a claim which must seem dubious even to a mainstream dialectologist. Writing of McDavid's own children, Davis laments:

> It would be fascinating if I could report that the offspring of a Minneapolis speaker [Virginia Glenn McDavid] and a South Carolinian [Raven McDavid] would show phonemic splits and mergers of a novel kind, but they speak the Standard Inland Northern of their peers (1972:xi).

So there it is: the mainstream dialectologist knows that children learn the language of their peers yet conducts research predicated upon the assumption that they learn the language of their parents. Labov, whose work in nine years has probably taught us more about dialects than that of the linguistic geographers in 39 years, once again reminds us of a fact that should be obvious to us all:

> If children's speech reflected their parents' dialects, we would not find any of the solid isoglosses which mark the regional boundaries of American speech. Whenever a family moved to a neighboring town, we would then find that the dialect of this area was diluted with the coming of the imported child, and the overall result of this mixing process would be no regional dialects at all (1971:214, n 22).

In view of the obvious fact that the influence of parents upon a child's dialect is less permanent than that of the peer group, to continue to emphasize in dialect investigations parental factors at the expense of what are ultimately more important ones does not seem to be justifiable.

The Interview

One of the claimed virtues of atlas methodology is "interviewing in a conversation" (Pederson 1972:8), but close attention to an atlas interview reveals that it more closely resembles a test than it does a conversation. Conversational forms were from the beginning of American atlas surveys regarded as "especially valuable" and "of great importance" (Kurath et al 1939:45) because they were believed to be more natural than answers to direct questions, which tend to provoke guarded responses. Because of this goal, atlas questionnaires merely list items or notions instead of questions, and each field worker is encouraged to develop his own elicitation techniques, but the very nature of the items often mitigates against conversation. Consequently, the frequency of conversational forms varies from field worker to field worker. Kurath observed that although some LANE field workers recorded "many conversational forms" Loman rarely did "since in his interviews he pressed forward energetically and rarely permitted the informants to expiate or to drift into story telling and anecdote (Kurath et al 1939:45-46). Since true conversation is inefficient

when a field worker is intent upon recording an informant's usage for 700 to 800 specific items,[5] field workers tend to

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[5] Conversation is not just inefficient; it is practically impossible. In early ALS pilot interviews we attempted to elicit both vocabulary and pronunciation items in a conversational setting. We had in this test version of the questionnaire a select list of vocabulary items and our goal was to record the informant's term for each "thing." Also, we had pronunciation items, and in good atlas tradition we were intent upon recording every speaker's pronunciation of exactly the same words--for reasons of comparability, naturally. Armed with this list, we set out to carry on conversations with Arkansawyers. Invariably one of two things happened. Either we carried on lively conversations which were unproductive in terms of our target list, or conversations quickly deteriorated into a question-and-answer session. The problem is simple: maintaining a conversation depends upon the interviewer's projection of genuine interest in what speakers talk about. When we let our speakers talk about topics that held interest for them, we discovered that rarely did they express interests in the semantic domains on our list. We also discovered that whenever we attempted to direct the attention of the conversation to areas of our interests, speakers sometimes simply ignored our ploys and continued to

resort to a procedure of eliciting one-word or phrase responses.[6] The result is, ironically, just the type of utterance Kurath wanted to avoid.

The atlas interview in which a field worker uses his verbal skills to elicit a response indirectly without using a possible answer in the question is notoriously inefficient. It wastes valuable time, and often imprecise hints result in erroneous answers. McDavid estimates that a LAGS interview, recorded on tape, containing 800 items can be expected to be completed by an experienced interviewer in two or three sessions of two to three hours each. Difficult informants or inexperienced interviewers will require even more time

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talk about whatever they thought was worth recording. At other times our attempts to direct the conversation created tension that resulted in guarded speech, terse responses, a drop in enthusiasm, and an increased suspicion of the field worker's motives. Van Riper claims to have conducted 50 interviews in Oklahoma in a conversational style (1972:177-183), and I have not had the opportunity to hear his tapes. But judging from our experience with the pilot ALS interviews, however, it is highly doubtful that Van Riper was eliciting casual, spontaneous speech.

[6] See Atwood 1963:11 and Pederson et al 1972:101-208 for examples of field worker techniques.

(1972:48). There is no need to prolong interviews to such lengths, and speed is easily attained by adopting other elicitation techniques. McDavid estimates that the maximum speed for an atlas interview is three questions a minute (1972:40). But in the ALS, in contrast, questions are used only as a last resort to elicit word or phrase utterances. By relying on illustrations the ALS procedure elicits 437 pronunciation features in less than 20 minutes (Underwood 1972:19); the same technique can be used not only for vocabulary, but also for morphology and syntax (Summerlin 1972).

Consider this sample attempt to get a response by Raven McDavid, who is generally regarded to be the most capable of the atlas interviewers:

> A long thin-bodied insect, with a hard little beak and two pairs of shiny wings; it hovers around damp places and eats its own weight in mosquitoes, etc. (Pederson et al 1972:161).

The desired response is <u>dragon fly</u> or a synonym, but the technique is counterproductive because McDavid, not the informant, does most of the talking. This is precisely why the ALS abandoned direct questioning; a study of early pilot interviews using direct questions revealed that our tapes contained usually 10 words from the interviewer for each word spoken by the informant (Underwood 1972:19). But more importantly, atlas-type questioning is simply misunderstood all too often. The item McDavid is trying to deal with, for example, is one--and there are no doubt

many others--which is not at all clear when the request for information is verbal, as Smith has convincingly pointed out (1968:51-57). But as Smith also shows, erroneous answers virtually vanish if an illustration is used instead of an ambiguous question. Put briefly, the process of eliciting answers with oral questioning is far more complex than atlas methodology assumes. Labov (1970b:56-59) describes some of the fundamental presuppositions an interviewer and a respondant must share before a question will evoke a desired response. The study of Schatzman and Strauss (1955) clearly demonstrates how a question, considered simple, obvious, and unambiguous to an interviewer, is understood with a wide range of interpretations by those being interviewed. It is not inaccurate to say that the question an atlas interviewer asks and the one his informant answers are often two distincly different requests. When that happens the informant's answer is appropriate to the question he understands, and since it may appear to the interviewer to be a legitimate response to the question he has asked, it is recorded erroneously as a valid response.

It should be acknowledged that Kurath has for at least 34 years been aware of the inadequacies of atlas interviewing techniques. He noted that "the fieldworker's method of interviewing...affects the informant's responses" (Kurath et al 1939:47). He went on to observe that "the wording of the items in the worksheets [Kurath's emphasis]...to some

extent determined the method of inquiry and hence also, in a measure, the response" (Kurath et al 1939:47). Since standardized questions are not used and the field worker is free to develop his own elicitation techniques, Kurath noted a tendency among field workers to use literary expressions for concepts for which the informant is expected to provide his natural synonyms. Such methods result in distortions of the evidence, as Kurath well knew. Despite these reservations, mainstream dialectologists continue to use the same elicitation techniques in the face of all the reasons for dropping them. Although there were claims for improved elicitation techniques (Pederson 1971:138), the LAGS work sheets are identical in character to all the previous atlas questionnaires, the only difference being the inclusion of illustrations of elicitation methods used by two LANE field workers (Bloch and Harris), Loman, McDavid, and Pederson (Pederson et al 1972:99-100). Kurath recognizes but plays down the inadequacies of atlas elicitation techniques by saying that the "approach may produce variants that are not strictly synonyms," but instead of attempting to develop a better methodology he merely excuses the procedure with the feeble conclusion that it has "drawbacks of which scholars must be aware" (1972:15).

I have dwelled at length upon what I consider to be the serious flaws in elicitation techniques used by mainstream dialectologists, but even if those flaws were elimi-

nated, the resulting data would still be unsatisfactory. The evidence would not reflect a true picture of a speaker's linquistic repertory; it would at best illustrate no more than his performance in one narrow, uncomfortable communicative context. A representation of a person's linguistic capacities that encompasses just one speech style is obviously incomplete, and, more importantly, sometimes conclusions based upon such evidence can be dangerously misleading. In their own way mainstream dialectologists are just as guilty of presenting a biased representation of a speaker's linguistic ability as are such researchers as Bereiter and Engelmann, who conclude that upon the evidence of their research ghetto youngsters are non-verbal, grammatically impoverished, and cognitively deficient (Bereiter et al 1966; Bereiter and Engelmann 1966). Unlike Bereiter and Engelmann, mainstream dialectologists do not misinterpret the evidence--make no mistake about that!--but like Bereiter and Engelmann they take evidence collected within the constraints of one situational context and regard it as indicative of a person's total linguistic capacity. Unwittingly, Kurath was throwing the baby out with the bath when he directed field workers to avoid "guarded" responses which may reflect a speakers consciousness of "correctness" (Kurath et al 1939:45).

In effect, the mainstream dialectologist is assuming a hierarchy of linguistic variation. Of primary importance are regional variables, of secondary importance are social

variables (as he understands them), and of no importance--to him at least--are stylistic variables. Yet despite his efforts to insure that data collected from an informant will be internally uniform, the mainstream dialectologist finds in his field records often so much free variation that generalizations seem impossible. However, Labov has demonstrated that what has been erroneously regarded as unanalyzable free variation is not truly free, that if the data base is expanded to include evidence recorded in a variety of contextual styles, the variation patterns regularly according to stylistic stratification (1966:28-62). Simply stated, if different contextual styles are built into the interview format we get a better idea of a speaker's linguistic capacity and a better account of linguistic variation. Clearly, the aspect of stylistic variation is an important consideration to dialectology, and it was encouraging to learn that Pederson planned to incorporate procedures for isolating different styles in the LAGS work sheets (Pederson 1971:138). Unfortunately, however, the latest version of those work sheets includes no such provisions (Pederson et al 1972:99-208). Instead McDavid instructs LAGS field workers that "the form of the interview should be kept as conversational as possible" (1972:45), meaning, in other words, that elicitation techniques are to be the same as those used without improvement since 1931.

Communities and Informants

According to Kurath the choice of communities to be studied by an atlas "is made in reference to the sociocultural characteristics of the area under consideration" (1972:11). Those characteristics include such factors as chronology of settlement, the characteristics of the population at the time of settlement, the development of railroad lines, early roadways, routes of migration as settlement expanded westward, and the character of the economy in past centuries (Kurath 1972:42-51). Once the settlement patterns are known, communities representative of this history are selected as cites for interviewing.

In each community an informant is selected who is "an elderly descendant of an old local family" (Kurath et al 1939: 41). Such old-fashioned people are important "in order that the earlier regional pattern might be accurately delineated and the oldest living forms of speech preserved as fully as possible for the historian..." (Kurath et al 1939:41). In addition, a middle-aged life-long resident, usually a high school graduate, is interviewed, and in each major city a cultured informant is also interviewed. These informants are classified as follows:

Type I: Little formal education, little reading and restricted social contacts.

Type II: Better formal education (usually high school) and/or wider reading and social contacts.

Type III: Superior education (usually college), cultivated background, wide reading and/or extensive social contacts.

Each type is also designated further:

Type A: Aged, and/or regarded by the fieldworker as old-fashioned.

Type B: Middle aged or younger, and/or regarded by the fieldworker as more modern (Kurath et al 1939:44).

Although the social classification system can be questioned, there is nothing wrong per se in choosing to interview such people. It is a legitimate enterprise if a person wants to study middle-aged and elderly elements of a stable population, people whose families can be traced to the earliest settlers in communities that are in turn typical of the migration patterns of the first European-Americans in the region. And this is not too far removed from what linguistic geographers do. McDavid reminds us:

> Whatever the current mode of interest in dialects may be, it is none the less true that the primary purpose of a linguistic atlas is that of historical linguistics, of providing a body of stable folk evidence, from which one may work backward, comparatively, to set up affiliations of the dialect regions with those in older settled areas and in the British Isles (1972:51).

Now that is a kind of candor to be admired, and it is a shame there is not more of it from mainstream dialectologists. Instead, they tend to condescend toward the "antiquarian bias" (McDavid 1966:12) of Europeans who have:

> a predilection for historical problems [in] the hope of shedding light on processes of linguistic change by observing the linguistic behavior of the folk (Kurath 1972:13).

They claim, in contrast, that American atlases yield a different sort of evidence with statements like this one:

> This systematic record of usage of more than 1,200 persons gives us full information on the geographic and the social dissemination of the words and the phrases selected for this study (Kurath 1949:v).

As Pickford pointed out (1956:217-218), it is not true that data from American atlases provide us with an accurate representation of the regional and social variations in American English. Obviously, if we want such a representation, a different methodology is needed.

II

In the preceding sections I have set forth and supported several arguments against mainstream dialectology. I have advocated that we not encourage or engage in such outmoded research in the Southwest. It does not follow, however, that no dialect research should be undertaken. On the contrary, we desperately need knowledge of language variation in both rural and urban areas, we need studies of the language characteristics of Blacks and of bilinguals (and even trilinguals in some New Mexico pueblos), and we also need to learn the correlation of linguistic variation with such social factors as a speaker's degree of urbanization, age, sex, social class, occupation, cultural isolation, and ethnic background. Such knowledge clearly requires a different methodology from that of the mainstream dialectologists.

The Questionnaire

We need to design a questionnaire which is basically model-oriented rather than item-oriented. Priority should be given to coverage of the phonological, morphological, and syntactic systems of American English instead of to vocabulary and incidental features. We should not be content with recording only a conversational or a recitation style of speech, but should instead design the research tool so that we record a person's speech in a wide range of contextual styles from the most casual to the most formal. The interview should also be broadened beyond the consideration of a speaker's productive competence to include evidence of his receptive competence as well, to use Troike's important distinctions (1969:63-73). This would necessitate inclusion of such matters as repetition tests like those used by Troike (1969:66-67) and Labov and Cohen (1967:12-16). Similarly, we need to develop techniques for eliciting reliable grammaticality judgments such as those used by Carden (1973) in his study of double negatives. Also the interview should collect data on social evaluations as well as grammatical ones. This would mean investigation of a person's linguistic self-evaluation and his evaluation of speech different from his own. Ideally the scope of the interview should be expanded beyond a speaker's linguistic competence to include his communicative competence as well. Finally, let's acknowledge that a questionnaire to be used

in the Southwest would have to be designed to deal with the presence of co-existent systems and bilingualism and in some cases trilingualism, and the problem is complicated further by the fact that a variety of bilingual situations exists. In Texas, for example, the questionnaire would need to account not only for Spanish interference in English but interference from German and French as well, and possibly even Czech, Polish, and other European languages. In other sections of the Southwest parts of the questionnaire would have to be adapted for numerous Indian languages. The construction of such a questionnaire will be no easy task; it is going to require the expertise not only of a team of linguists familiar with the languages spoken in the area, but also of anthropologists, sociologists, and folklorists who are knowledgeable of the various cultures in the region. Indeed, the experience of Galvan and Troike has demonstrated the indispensable role of non-linguists in an area of relatively uncomplicated cultural diversity such as East Texas (Galvan and Troike 1969:29-31).

Interview Techniques

It is obvious that the proposed character of the linguistic interview will necessitate refined interviewing techniques. As I argued earlier, atlas methods must be adandoned in total, and new procedures for eliciting speech must be developed that exploit the very best of the work of such researchers as Sapon 1957, Labov 1966, Labov, Cohen,

Robins, and Lewis 1968, Bachman 1970, Underwood 1972, and Summerlin 1972, to name only those that come to mind. The extension of the questionnaire into new domains--new for the dialectologist, at least--also means that we will no doubt want to turn to methods used by other kinds of linguists and non-linguists as well. Just one example of this type of borrowing is suggested by Wolfram, who proposes that help in exploring differences in grammatical systems may come from those who have been studying children's acquisition of language (1971:121). It may not be enough, however, just to train a field worker in new sophisticated techniques. We need to learn whether or not the interviewer's race, sex, or social class, for example, affect the performance of the speaker. There is reason to believe these factors are important, for Anshen's data, to use one example, suggest that the race of the interviewer affected the pronunciation of some of his Black subjects in North Carolina (1970:21). Because of the complex linguistic and cultural situations in the Southwest, the interviewer may be an important constraint upon variability. Before a survey of linguistic variation is begun, we first need to conduct experiments to learn the significance of what Wolfram has called "interviewer variables" (1971:120).

Communities and Informants

If a study of linguistic variation in the Southwest is to give an accurate reflection of the English of the

population, it is obvious that atlas methods for selecting communities and informants are not viable. A refined sampling methodology has distinct advantages over judgment sampling not because it is free of bias but because of all its biases are explicit, and there are empirical tests for sampling error. In addition to the superior validity and reliability of such sampling, evidence obtained by such techniques can be subjected to a wide range of statistical calculations, whereas that from judgment samples cannot. But devising a feasible methodology poses problems which the linguist alone cannot solve.

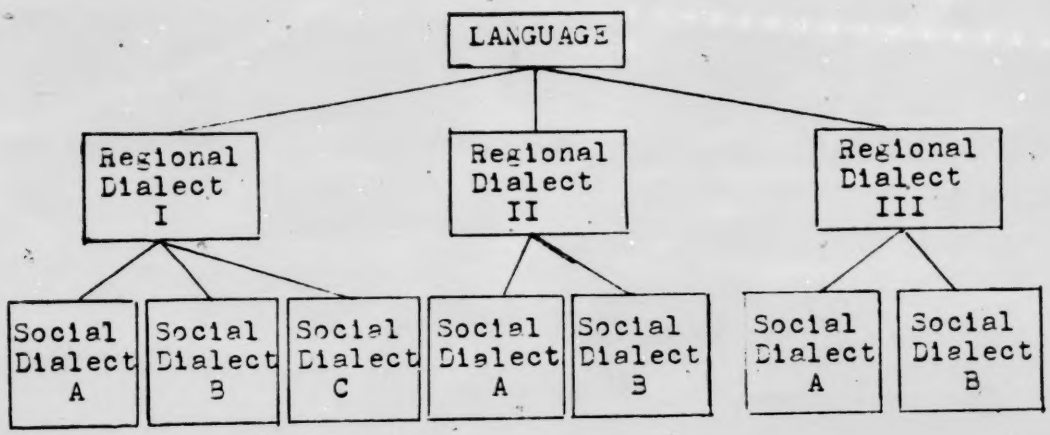
Let's consider some of the complications present in Texas alone. First, the population in 1970 was 11,196,730, of which 79.7% was urban. Suppose, therefore, a sample was sought that reflected this rural-urban proportion. It may not be the best sample because there is reason to hypothesize that the dialect variation (and social stratification) in the "Anytowns," i.e. towns ranging in population from 2,500 to 50,000 (Hodges 1964) is different from that of metropolitan areas. Galvan and Troike (1969:31), for example, note a variation in Tyler not found in four smaller East Texas towns. In Texas there are 24 such metropolitan areas comprising 73.8% of the total population, leaving only 20.3% of the population in rural areas and a mere 5.9% in towns outside metropolitan areas. Furthermore, over 12% of the population is Black and over 16% is

Spanish-surnamed, but these minority groups are not evenly distributed in the state (see Figure 1). If we assume a hypothesis that dialect differences correlate with cultural boundaries, we first must ask what are the cultural divisions of Texas. Suppose, for a purely hypothetical example, a sociological survey divides the state according to Figure 2. In areas 1, 2, and 4 the Black population ranges from 15% to over 50% in some counties, and in areas 7 and 9 the Spanish-surnamed population varies from 15% to over 75%. Note, too, the shaded areas indicating metropolitan areas. Presumably the sample would have to be extensive enough to represent adequately the population character of each region, but we do not know the minimum sample that would be adequate. The sampling problems in Texas alone are so mind-boggling that one is reluctant to think about those in other Southwestern states.

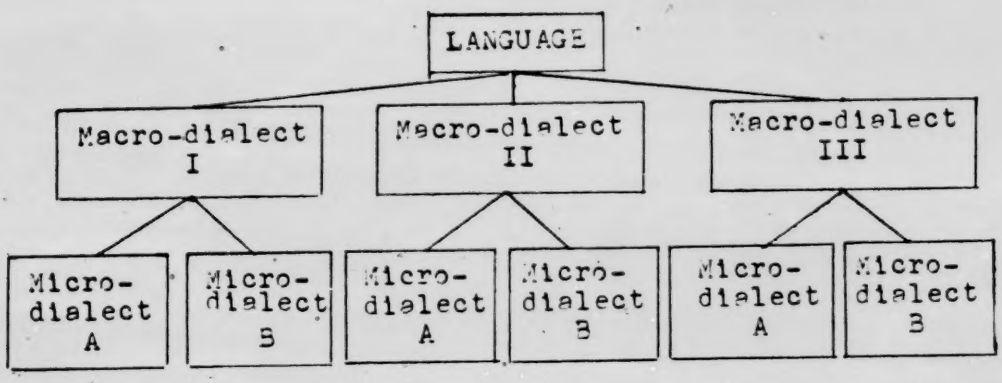
III

Some people are certain to observe that what I am proposing as a replacement for traditional, mainstream dialectology is nothing more than social dialect research conducted over a large geographic area instead of in an urban area only. That is a fairly accurate observation, but there is no reason why the same methodology should not work in regional studies that works in urban areas. The dichotomy between regional dialects and social dialects is a false one anyway. Traditionally, dialectologists construct a dialect model

looking something like this:



But if a dialect is a variety of the language spoken by a group of speakers, differing from other varieties of the language by systematic differences in language structure, there is no requirement that those differences must be geographically distributed. Furthermore, there is no reason to assume that differences distributed geographically are inherently more significant than those with non-geographic distribution. In fact, if language variation is a social phenomenon, if it is a variation to be accounted for in terms of social factors such as age, sex, ethnic groups, social class, place of residence, cultural isolation, etc., as Dillard (1969) insists--in other words in terms of what a sociologist calls group identification and affiliation, then there is no reason to assume on an a priori basis that any of these factors necessarily outranks the other. If models are desired, a more realistic one would be:



Such a model makes no claims about social or regional distributions; it merely suggests that macro-dialects I, II, and III have certain fundamental differences in their systems. Micro-dialect IA shares these fundamental differences with micro-dialect IB but differs from IB in more superficial characteristics. But it must be kept in mind that such models probably have little relation to reality. They satisfy the linguist's predilection for abstractions, but their static character necessitates a ruthless segmenting of a linguistic continuum. If dialect research is to become model-oriented, our task will be to describe linguistic variables and to correlate those variables with social ones. The type of dialect research I envision for the Southwest not only calls for a combination of difficult field work and theoretical models, but also for cooperation between linguists of various talents and anthropologists, folklorists, sociologists, and perhaps psychologists. Even this tentative and crude sketch of the suggested research poses such formidable problems that

the likelihood that such research will even be conducted may be extremely remote. That is no justification, however, for maintaining the status quo of mainstream dialectology.

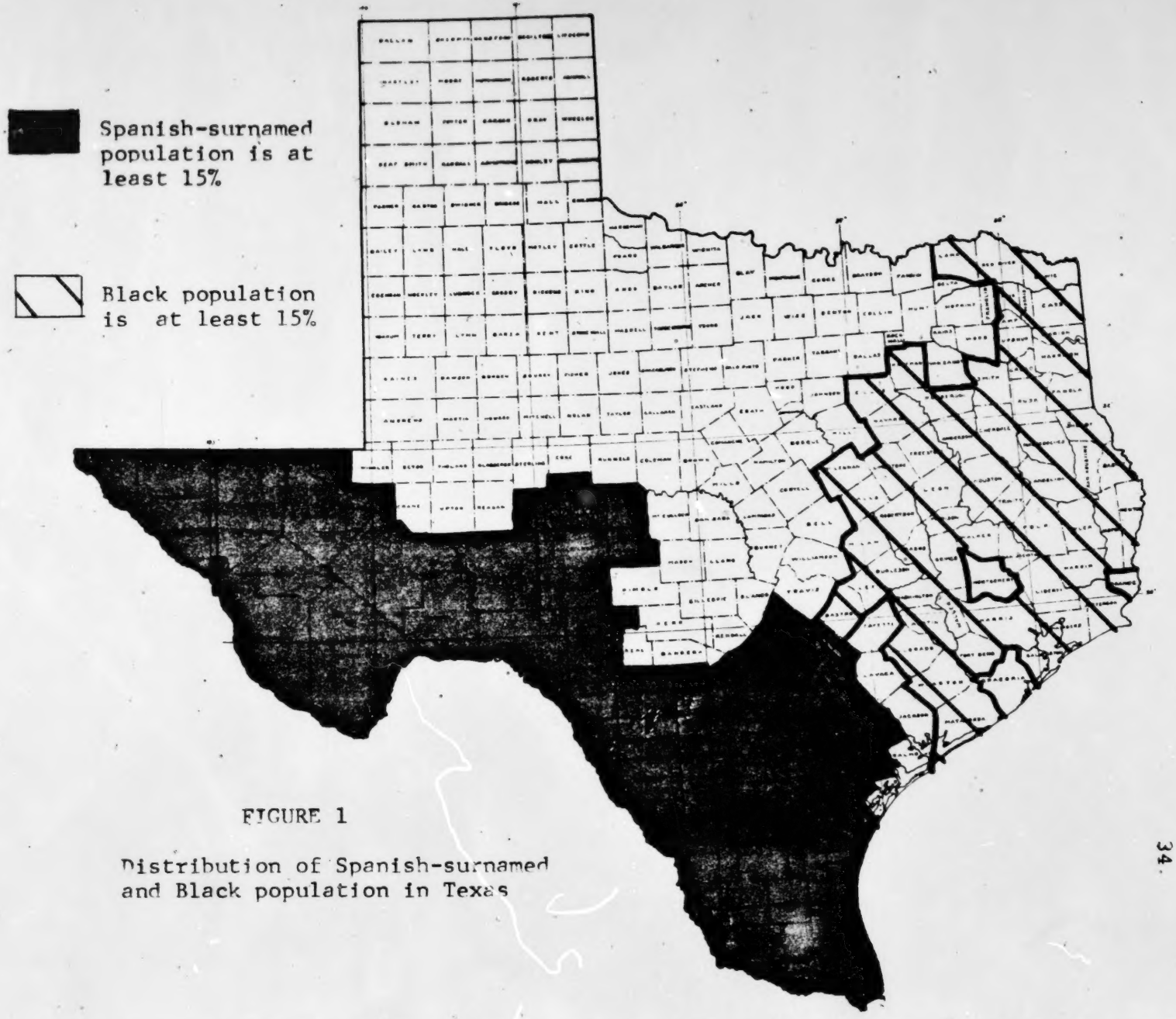


FIGURE 1

Distribution of Spanish-surnamed and Black population in Texas

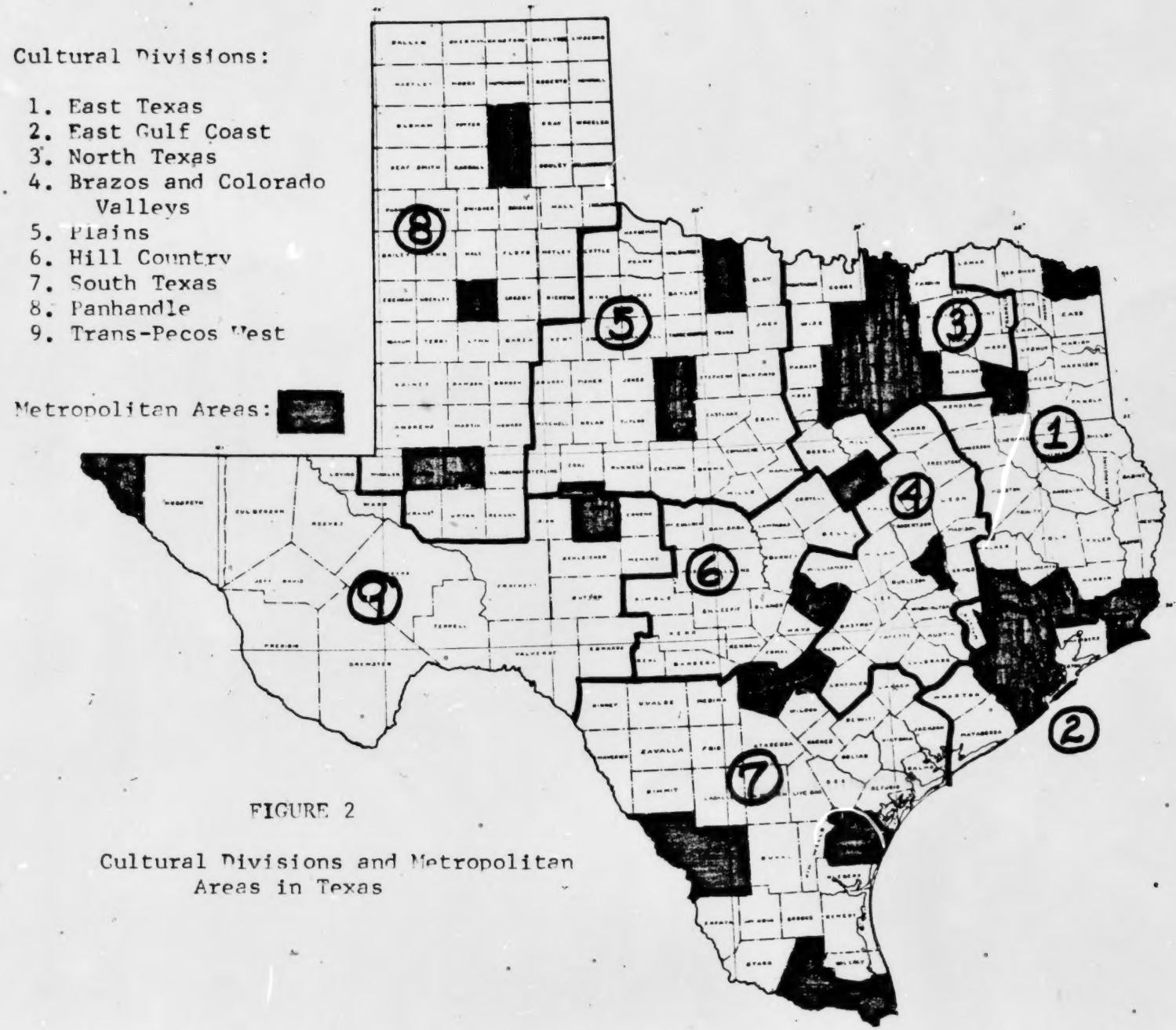


FIGURE 2

Cultural Divisions and Metropolitan Areas in Texas

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